

OPEN LETTERS.

A REPLY.

AT the close of a review of one of my papers in the GAZETTE, January 1898, p. 67, there is a statement which reflects on me in such a way that silence would virtually imply admission of the charges. I am therefore obliged, albeit reluctantly, to make answer. This statement is as follows: "There is a curious omission of reference to the researches of Dr. H. L. Russell on this disease, some of which have already been published, as well as those unpublished, of which Dr. Smith was fully cognizant. We reserve further comment on this matter until the publication of Dr. Russell's paper, of which advance sheets have reached us." In general it is wisdom to hear both sides of an argument and to know something about the merits of a case before pronouncing judgment. I am satisfied that had the reviewer taken this ordinary precaution the above criticism would never have been written, and certainly never printed. Under the circumstances, I must beg to make an explanation.

My own studies of the parasite which causes the black or brown rot of turnips, cabbages, and allied plants, began in the fall of 1896, and have been continued uninterruptedly to date. During this time I have alluded to the subject or spoken at length upon it in seven public addresses, viz., before the Massachusetts Horticultural Society in Boston, March 1897; before the Washington Botanical Seminar early in 1897; before the Washington Biological Society, May 1897; before Section G. of the A. A. A. S. at Detroit, August 1897; before the Rochester Academy of Sciences, October 1897; before the Society for Plant Morphology and Physiology at Ithaca, December 1897; and, finally, before the Peninsula Horticultural Society at Snow Hill, Md., January 1898. Notices of a number of these addresses went into newspapers and journals. During the same time I have published three articles on this organism: first, an abstract of the address before the Biological Society of Washington;¹ second, a long paper in the *Centralblatt für Bakteriologie*, published in three parts, July 7, August 18, September 10, 1897;² and finally, from the U. S. Department of Agriculture, a Farmers' Bulletin,³ describing the results of field studies and showing how the disease

¹ Science, 5 : 963. June 18, 1897.

² 3² : 284, 408, 478. *pl. 1.*

³ No. 68, January 8, 1898.

may be prevented. For the information set forth in these lectures and papers I am in no way indebted to Dr. Russell.

The paper reviewed in the GAZETTE was sent to the *Centralblatt* early in the spring of 1897. Proof on the whole of it was read and returned May 4. Some reprints of the paper were distributed in this country September 29 and the rest October 23. Up to the date of this writing (January 28) Dr. Russell has, on the contrary, so far as I know, not published any important information respecting this parasite *Pseudomonas campestris* (Pammel), in fact, he did not seem to know of Professor Pammel's paper until I called his attention to it. Whatever "advance sheets" the reviewer may have seen, I have not seen any, neither has the general public, nor do I know what their contents may be. The expression, "some of which have already been published," probably alludes to a paper by Dr. Russell, which was read at the Springfield meeting of the A. A. A. S. in August 1895. I was not present at that meeting and never learned orally or through writing as to the contents of this paper. I desired very much to read the paper, but it was withheld from publication, and the only abstract of any value which I have been able to find is in the Proceedings of A. A. A. S. 44 : 193. 1895. I might have alluded to this short abstract and would have felt compelled to do so had I been considering cabbage diseases in general, rather than writing a paper on a particular organism. This abstract I read carefully a number of times, but never found anything in it which in any way aided me in my investigations. The symptoms of the disease are not carefully described, it had not been produced by inoculations with pure cultures, and the author was evidently in error as to the common natural methods of infection as the following citation shows: "The disease is first noticeable in the axil of the lower leaves in the sulcus on the upper side of the petiole. This depression is usually filled with moisture and the disease often gains entrance at this point through the mechanical rents that are caused by the rapid growth of this succulent tissue." On the contrary, the disease due to the organism which I have studied is generally first noticeable at the margins of the leaves. He also stated that his rot "spreads rapidly in the loose cellular parenchyma" of the petioles, which is not true of *Pseudomonas campestris*. Finally, there is not a line as to what kind of an organism he was experimenting with. Subsequently Dr. Russell told me (November 1896) that he was at that time (1895) working with the "wrong germ," namely, with a white organism, and this probably explains why he withheld the paper from publication, and also explains why I preferred to pass over the abstract in silence.

Now, as to what I have learned orally or by correspondence with Dr. Russell, or from other people, concerning his "unpublished" work, so as to be "fully cognizant" of it. The whole thing can be put in a nutshell. How I first came to undertake the study of this organism is sufficiently set

forth in the first part of the paper reviewed. Not until my investigation was well under way and I had asked for and had received a second shipment of cabbages from Racine did I know that samples had also been sent to Dr. Russell, and that he had again undertaken to find out the cause of the disease. At no time have I visited his laboratory or seen any of his cultures or any of his experiments, or had any desire to know what he was doing. In November 1896, Dr. Russell visited my laboratory desiring, as he said, "to talk shop," or, in other words, to learn what I was doing. Some of his questions I answered, others I parried, as any other man would have done, not desiring to give away to another working in the same lines information relating to an unfinished piece of research. At that time he said he had secured no infections and was unable to get the organism to grow in beef broth. He obtained from me a few facts which probably were of use to him, namely, that I was still working on the disease, that my organism was yellow, and that it would grow in properly made beef broth. From him I received the statement that the organism which he was then studying was yellow, the other statements which I have given above, and the fact that the loss at Racine exceeded \$75,000. I obtained from him no facts which in any way changed my plan of work, and no ideas which were of any value to me except the statement as to the approximate pecuniary loss at Racine, which statement I carefully refrained from using, depending rather on general statements furnished by cabbage growers. If Dr. Russell had any ideas at this time as to the mode of infection or other interesting peculiarities of the organism, they were not revealed to me.

This is all I knew definitely of Dr. Russell's work until ten months later (September 1897) after the publication of two-thirds of my paper and within a week of the appearance of the remaining part. He then informed me that he had also secured infections and was preparing a paper for publication. He volunteered, however, scarcely any information concerning the details of his work, and no information whatever of which I have made any use, neither did I ask for any, nor desire any.

Such are the sole grounds for the charge of omission to give credit for work already done, and of being "fully cognizant" of unpublished work. Dr. Russell, as I learned from a conversation with him in September 1897, seems to have gone away from Washington with the idea that I would drop work on the germ, but I did not designedly or intentionally give him any such impression.

It has always been my desire to give all work the fullest recognition. If any one has cause to accuse me of sins of omission, it is not Dr. Russell but Professor Garman, who published a paper three years before the reading of Dr. Russell's Springfield paper (*Agricultural Science*, July 1892, pp. 309-312) on a bacterial disease of cabbage, and which, if it did not go very far, is at least cautious, covers much the same ground as Dr. Russell's abstract, and is

a much more useful contribution to science. I omitted mention of this paper because no infections were obtained from pure cultures, because the two organisms which were isolated were not described so as to be identifiable, and finally, because I was describing a specific germ and not writing a general treatise on cabbage diseases of which there appear to be several due to bacteria. The one man who has written on this organism so that other bacteriologists can make something out of his writings is Professor L. H. Pammel, to whom I gave full credit. Since the paper in question was written I have removed the final shadow of doubt respecting the identity of the organism which Professor Pammel described and the one I have studied, by the discovery that whether it *does* or *does not* liquify gelatin is an accident depending entirely on how the gelatin is made.—ERWIN F. SMITH, *Washington, D. C.*

[WE PUBLISH the above at Dr. Smith's request. Dr. Smith could hardly have written it had he awaited the promised comments when Dr. Russell's paper was noticed (see p. 211). The reviewer was cognizant of the facts which Dr. Smith relates above (and some others) when the statement he quotes was written, and sees no occasion to change his judgment. It was as far from his thoughts then as now to charge Dr. Smith with improper use of information; nor can such an implication be found in the review. Dr. Smith is certainly entitled to the full credit of independent work. The complaint was rather that it was *too* independent (witness the allegation, "at no time have I . . . had any desire to know what he [Russell] was doing"); and we can see no explanation of the studious avoidance of any reference to the antecedent and contemporary work of Dr. Russell, of which, by his own statement, he *was* "fully cognizant." We leave readers to judge of the validity of the reasons assigned for passing over it in silence. We only remark that, under the circumstances, Dr. Smith had every reason to know that the disease described by Russell, even though imperfectly, was identical with the one he was working upon. Nor does he improve his relation to the matter by magnifying now the errors of that preliminary notice. In newspaper parlance the earlier publication of important information constitutes a "scoop," and in newspaper rivalry, to "scoop" a competitor is not only legitimate, but praiseworthy. It is hardly so regarded in scientific work.—EDS.]

BOTANY AT BROWN UNIVERSITY.

To the Editors of the Botanical Gazette:—I think it is my privilege to correct a wrong impression likely to be conveyed by the recent notice of my *Notebook* in the GAZETTE. Personally I am wholly indifferent to the criticism, but as my university is involved in the charge of erroneous teaching, I feel bound to say a word in her defense.

It is said in the review that a glimpse is here given of the mode of teaching botany at Brown. A glimpse, yes; a comprehensive view, no. My *Notebook* is designed wholly for primary students and has been found to meet its purpose admirably well. Histology and the lower cryptogams,